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Kuhn and the Chemical Revolution: A Re-assessment.

Abstract.

A recent paper by Hoyningen-Huene argues that the Chemical Revolution is an excellent example of the success of Kuhn's theory. This paper gives a succinct account of some counter-arguments and briefly refers to some further existing counter-arguments. While Kuhn's theory does have a small number of more or less successful elements, it has been widely recognised that in general Kuhn's theory is a "preformed and relatively inflexible framework" (1962, p. 24) which does not fit particular historical examples well; this paper clarifies that those examples include the Chemical revolution.

Introduction.

In a recent paper, Hoyningen-Huene (2008) argues that the Chemical Revolution is an excellent example of the success of Kuhn's theory. In the context of the forthcoming fiftieth anniversary of the 'Structure of Scientific Revolutions', it is appropriate to appreciate not only the very wide circulation and considerable influence of Kuhn's work, but also the near-zero use of his detailed theories in subsequent history of science. The most notable example of the latter was Kuhn's own "Black-Body Radiation" study (1978), in which the term 'revolution' is used in largely the same manner as in Kuhn's early "Copernican Revolution", and without the detailed apparatus in SSR. When questioned about this, Kuhn's (2000, p. 314) excuse was that he did not use the philosophical apparatus when doing history. However, it has been widely recognised that in general Kuhn's theory is a "preformed and relatively inflexible framework" (1962, p. 24) which does not fit particular historical examples well; this paper succinctly details the lack of fit between Kuhn's theory and the history of the Chemical Revolution.

Crisis.

The basic problems with Kuhn's and Hoyningen-Huene's accounts are that the actual crisis in the Chemical Revolution happened in 1785 which is much later than they suggested, and the crisis came *after* the production of the new theory, not before it. Perrin's classic paper (1988) on these topics clarified (pp. 110-113) three phases of publications by Lavoisier and of relations between him and the remainder of the French chemical community. The first phase centred on his *Opuscles* (1774), which contained only minor dissent from Stahl's theory; this work enhanced Lavoisier's reputation among the established French chemists, with whom at this stage his relations might be described as cordial. The crucial publication of the second phase was the paper of 1777 on combustion, which included the well-known conditional proposal that *if* Lavoisier should succeed in accounting for combustion without using phlogiston, *then* Stahl's theory would be "shaken to its foundations"; after this, the reception of Lavoisier's views "turned cooler", and Lavoisier for the next six years "tacitly avoided confrontation with Academic chemists" over phlogiston (Perrin 1988, p. 112). It was only after news of the discovery of the composition of water in 1783 allowed Lavoisier to complete a reasonably full theory that he adopted a more aggressive stance, culminating in dramatic fashion in his 1785 paper 'Reflections on Phlogiston' which concluded that "Stahl's phlogiston is imaginary ... (the phlogiston) doctrine ... is ... more of a

hindrance than a help for extending the fabric of science” (Lavoisier, 1785, p. 655). It was after this that there was a general sense of crisis concerning the phlogiston theory among the French chemists (Perrin 1988, p. 113), and a quarrel between the phlogistians and Lavoisier’s antiphlogistians which Priestley claimed had aroused more “zeal and emulation” than anything else in “all the history of philosophy”, resulting in the recognition of a revolution in chemistry which Priestley (1796, p. 1) stated had been great, sudden and general.

However, Kuhn and Hoyningen-Huene proposed that the crisis happened by the mid-1770s. Kuhn’s (1962, pp. 70-1) proposal for a crisis had the following elements: (1) although none of Black, Cavendish, Priestley and Scheele suggested that the phlogiston theory should be replaced, they were unable to apply it consistently; (2) there was the problem of weight-gain during calcination, although Kuhn explicitly recognized that this “did not result in the rejection of phlogiston theory, for that theory could be adjusted in many ways”; (3) Guerlac’s (1961) book on Lavoisier in the year 1772 in its entirety documented “the evolution and first recognition of a crisis”; and (4) “Priestley’s publications had revealed the full extent of the crisis in pneumatic chemistry” (Kuhn 1962, p. 86). However in last case, Priestley’s work is devoid of references to or intimations of crisis – he remained the most committed of the phlogistonists. Hoyningen-Huene’s (2008, pp. 106-8) version of a ‘crisis’ is significantly different. He concentrates only on weight-gain in calcination, and does not fully take into account Kuhn’s correct recognition that weight-gain did not result in the rejection of the phlogiston theory because the theory could be adjusted in many ways. He brings in the possibility that some of the results could be interpreted without the supply of phlogiston, and does not take into account that the results were still interpreted by phlogistonists within phlogiston theory (c.f. Pyle 2000, 106). He then simply asserts that “by then, in the mid-1770s, the crisis was in full swing”. According to Hoyningen-Huene, the ‘crisis’ extended well into the 1790s, but he also noted that according to Kuhn, ‘crisis’ should end with the production of a new theory

Kuhn’s general view was that a crisis would be followed by competing schools, attacks and extraordinary science, leading to a new theory. However, in this particular case, Kuhn’s view involves a notion of ‘crisis’ which was not recognised by all but one of the participants. It will be obvious that a principal problem with an idea of a ‘crisis’ that is not recognised by nearly all the participants, is that they will not go on to consequently form competing schools nor undertake attacks nor have any conscious involvement with extraordinary science, as will be seen in following sections.

Competing schools.

During the Chemical revolution, competition between rival schools of chemists happened from 1785, not before that time. As Fourcroy (1797 v. 3, p. 541) noted, “from 1777 to 1785, in spite of great efforts and numerous memoirs of Lavoisier, he was truly alone in his opinion”. This testimony is of particular importance in that Fourcroy was one of the group of young assistants who after 1777 met Lavoisier once a week for a day of experiments. Whereas Poirier (1998, p. 108) called this group the ‘Arsenal school’ and Perrin (1981, p. 41) argued that on this basis Lavoisier was not as alone as Fourcroy suggested, Fourcroy stated that the assistants aided Lavoisier without leading nor guiding him; he only used assistants to execute matters which he actually “started, pursued and finalised” (Bensaude-Vincent and

Abbri, 1995, p. 4). The assistants didn't even publicly support his view let alone compete alongside him until after 1785; Fourcroy then joined the team producing the nomenclature and from 1796 he focussed his lectures on Lavoisier's theory, while Lavoisier in effect insulted De la Métherie by delegating the job of replying to him to Adet and Hassenfratz (Perrin, 1981).

Perrin's (1988, p. 118) proposals for 'converts' to Lavoisier's views before 1784 are dubious. Perrin claimed Trudaine de Montigny as a convert before 1777 – but as Perrin noted he kept his views to himself. Perrin also claimed Bayen as a convert; however, he never acted together with Lavoisier's group and specifically acted against Lavoisier by getting Jean Rey's book republished. Perrin listed Bucquet as a convert for 1777-78; however he became ill in 1779 and died in 1780 so this had little impact. Accordingly it remains reasonable to date the commencement of 'competing schools' during the Chemical revolution to 1785, *after* the new theory was substantially in existence.

Attacks.

The main issue concerning attacks is whether Lavoisier's 1777 paper on combustion is to be counted as one. Conant (1950, p. 53) who wrote before the matter was crucial to a theory, deemed that Lavoisier did not attack until "Reflections on Phlogiston". A number of writers before Perrin, such as Musgrave (1976), counted the 1777 paper as an attack. However, the conditional wording of the main claim in the paper was significant; in military terms this claim was analogous to a warning shot or to 'military exercises close to a border', but the claim can reasonably be regarded as having been specifically worded so that it could be held not to constitute an attack. The response of the phlogistonists was fully appropriate in diplomatic terms – that is, it involved a cooling of diplomatic relationships rather than a counter-attack.

Lavoisier's paper on phosphoric acid of 1780 went a little further, putting forward that phlogistic explanations rested on unprovable assumptions, and suggesting that phlogiston was not a necessary concept. However, while this wording was more definite, these reservations were not necessarily an attack – after all, the same reservations could have been made about Lavoisier's caloric. The phlogistonists do not seem to have seen any resulting need to alter their cool diplomatic stance.

It follows that the attack is most reasonably seen to have occurred when contemporary sources thought it occurred, that is, in "Reflections on Phlogiston". However, even if an attack were deemed to have occurred in 1777, this would not take away from the difficulties that there was no general perception of crisis among French chemists until eight years later, and there were at the time no 'competing schools'.

'Extraordinary', 'pre-paradigmatic' and 'normal' science.

In Kuhn's detailed schema, science divided into three categories, 'normal', 'extraordinary' and 'pre-paradigmatic'; 'extraordinary' science (1962, p. 89) was proposed to happen only when a 'crisis' has occurred. 'Normal' science only involves collecting a restricted range of facts about already-identified phenomena: "no part of the aim of normal science is to call forth new sorts of phenomena" (1962, pp. 24-5); 'normal' science is based on past achievements that are recounted in textbooks that expound accepted

theory and illustrate many of its successful applications and compare those with exemplary observations. 'Pre-paradigmatic' science (1962, pp. 10-22) is an early phenomenon, when science has no established procedures, achievements nor theories.

However, neither phlogistic science nor Lavoisier's work fits properly into any of these categories. The main experimental activities and discoveries during this period continued an existing experimental tradition by the phlogistonists. Black had earlier identified a specific type of gas which he called 'fixed air' and Cavendish (1766) identified another specific type of gas which he called 'inflammable air'. Scheele in 1771/2 (although he did not publish till 1777) isolated a new type of gas which he called 'fire air'; Bayen isolated the same type of gas in April 1774, as in August 1774 did Priestley who called it 'dephlogisticated air', and he was also the first to identify that the newly-isolated gas was more fit for breathing than ordinary air. Scheele was the most prolific discoverer of new phenomena at this period, predicting and then finding whole families of hitherto unknown acids which could be produced simply by the 'dephlogistication' of non-metals, both inorganic and organic; Priestley's further research successes included the successful reduction of metallic calces, especially that of lead, with 'light inflammable air'. Cavendish discovered or confirmed the product of the sparking together of inflammable air and dephlogisticated air in 1783. Lavoisier's main experimental contribution had been on March 29th 1773, when he observed that lead calcining in a closed retort stopped doing so after a time, and hypothesised that contact with a circulating air is necessary for calcination, and that only a portion of atmospheric air enters into the metals being calcined. In this sequence of cases, there is no cut-off point following which the experimental science qualifies as extraordinary. The successful experimenters nearly all perceived themselves to be working within the phlogiston tradition rather than in an extraordinary or crisis-response manner, and they worked without extraordinary laboratory methods – there was no significant difference between the experimental discoveries in the phlogiston tradition before (Black, and Cavendish in 1766) and after Kuhn's proposed crisis point. Hoyningen-Huene argued that Lavoisier was the first to systematically weigh chemical reaction partners before and after the reaction in a closed system. However, Kuhn's own more equivocal view followed an excellent summary of the topic given by Guerlac (1961, p. xv), which included the fact that Cavendish used the balance with greater accuracy than Lavoisier did. Guerlac (1961, p. xvi) noted that Lavoisier used the balance with 'fidelity and persistence', and this proved important even though he did not always use it with 'rigorous accuracy'; however, this difference scarcely deserves to be labelled extraordinary. Additionally, others of Kuhn's criteria for crisis were also absent during the interim period prior to 1783 – there was no explicit discontent among phlogistonists with the core theory, no debate over fundamentals, no recourse to philosophy.

Kuhn (1962, p. 15) identified that chemistry was at the 'pre-paradigmatic stage' prior to Boyle and Boerhaave. Phlogiston meets a number (though not all) of Kuhn's (1962, p. 17) paradigm criteria, for example the triumph of a single school, which "because of its own characteristic beliefs and preconceptions, emphasised only some special part of the too sizeable and inchoate pool of information"; prior to Lavoisier, phlogiston had indeed achieved single theory status, (Kuhn, 1962, p. 72). It is also reasonable to argue that there was some 'core theory' to phlogiston theory; one version of this might include (a) that the apparently distinct phenomena of combustion, calcination and respiration had a common explanation, (b) that phlogiston was the 'principle of inflammability' and the principle of

‘reducing power’, (c) that this principle could be transferred from one combustible or reducing agent to another, and (d) that metals consisted of calces combined with phlogiston.

However, it is also reasonable to suggest that phlogiston was several criteria short of a paradigm. Lavoisier (1785, p. 640) stated that the phlogiston principle was so vague and undefined “it can be adapted to any explanation one wants to give for it ... It is a veritable Proteus that changes form every second”. Kuhn and Hoyningen-Huene argued that adaptations of the phlogiston theory constituted proliferation associated with a crisis, but the problems with such a view of crisis have been set out above; Pyle has noted that proliferation had been a feature of phlogiston theory from its early days. With regard to other Kuhnian paradigm criteria, it is difficult to identify exemplary problems and their phlogistic solutions. There would be also be difficulties with identifying Kuhnian ‘normal’ science at this period: Kuhn suggested that Priestley’s research on gases emerging from solids was normal science – however, this programme does not properly meet Kuhn’s stipulation that normal science does not aim to find new sorts of phenomena. Phlogiston theory in general was associated with copious discovery of new phenomena, was not occupied with a restricted range of ‘puzzle-solving’ concerning pre-identified phenomena, and did not meet all the criteria for a paradigm. By contrast, much of Lavoisier’s experimentation was aimed at finding more precisely a restricted range of facts about known phenomena, and would thus fit better into Kuhn’s ‘normal’ science category, even though it was designed to support a radical new theory and even though according to Kuhn’s theory Lavoisier’s experimental activity at this stage should be ‘extraordinary’.

Accordingly, if the Chemical revolution is viewed through Kuhn’s framework including his categories of science, this once again produces distortions – in actuality, neither the activities of the phlogisticians nor those of Lavoisier fit properly into any of Kuhn’s three categories.

Unexpected discoveries.

Hoyningen-Huene (2008, p. 109) argued that the discovery of oxygen constituted the unexpected experimental discovery that Kuhn’s schema proposed would accompany a revolutionary phase of science. This argument is explicitly contrary to Kuhn, who (1962, p. 56) had already identified that the discovery of oxygen was expected by Lavoisier, since it filled a slot in a hypothesis that he had already produced about the composition of atmospheric air; Hoyningen-Huene here does not take into account Lavoisier’s theory published in his ‘Opuscules’ in January 1774, which preceded the discovery of oxygen by Bayen and Priestley. In addition, it has been seen above that the discovery of oxygen was made by phlogistians, did not occur in science that can accurately be called ‘normal’, ‘pre-paradigmatic’ nor ‘extraordinary’, nor was it a consequence nor a cause of a crisis. A better candidate for unexpected discovery would be the nature of the composition of water. This was indeed unexpected to Lavoisier, (who had expected the oxide of ‘inflammable air’ [hydrogen] to be an acid). However, it did not involve extraordinary experimental science, was made by a phlogistian, Cavendish, (not by an adherent of the new system) and was not made in response to a ‘crisis’.

The sequence of the Chemical Revolution.

It is now possible to outline the differences between Kuhn's proposed cyclical sequence for scientific change, and what actually happened in the Chemical Revolution. Kuhn's sequence may be represented as:

S_K Serious anomaly → crisis → extraordinary science → accepted new theory
 → competing schools →
 → unexpected discovery →
 → attacks →

However, the actual sequence may be represented (with the noteworthy absence of extraordinary science) as:

S_L one scientist → sabre → Full new → dramatic → sense of crisis → competing schools
 finds start rattling theory full attack → further attacks
 of new theory (deprecated)

Whereas Kuhn sought a sequence that would apply to all scientific revolutions, the actual sequence for the Chemical revolution differs from those for other revolutions. For example, whereas Lavoisier provoked a crisis in order to forward his revolution, Copernicus tried to minimise the revolutionary appearance of his theory in order to forward its acceptance.

Kuhn-losses, the standards required for scientific explanation, and epistemological status.

According to Kuhn, the new theory emerging from a scientific revolution would have some areas in which it would not succeed in matching the explanatory success of the old theory, resulting in so-called Kuhn-losses. He suggested that in the case of the Chemical Revolution, the loss was in the explanation that phlogiston-content provided for the common characteristics of metals, and that this explanatory power was not recovered until the much later notion that metallic properties are to do with a ‘sea’ of free electrons. He (1962, p. 107) leaped from this to the idea that the new theory could not explain chemical qualities in general; he then alleged that this led to a change in standards of explanation so that “failure to explain the qualities of compounds was no indictment of a chemical theory”. However, phlogiston was supposed to also be a common constituent of sulphur, phosphorus and all combustible bodies; accordingly, phlogiston would not provide the explanation of the unique properties of metals. If whatever properties phlogiston had explained were to be revived with the notion of a sea of free electrons, the latter would also need to be a property of sulphur, phosphorus and all combustibles. Accordingly, this is not a tenable parallel, and there is no Kuhn-loss in this respect. There is also no historical evidence that there was a change in standards such as Kuhn suggests; Kuhn quoted Meyerson on this, but Meyerson (1930, p. 230) had not given any evidence.

Kuhn originally proposed that statements that were seen as empirical and possibly controversial before a scientific revolution, may be seen as analytic and trivial thereafter. Hoyningen-Huene adds that they may alternatively be seen as circular before and empirical after a revolution. His example was Lavoisier's view

(in his 1777 paper) that the phlogistic explanation of combustion was circular – the burning of combustible bodies was explained by their content of phlogiston, but the existence of phlogiston was based on the combustibility of bodies. However, it is reasonable to propose that Lavoisier's objection required doublethink on his part, in terms of forgetting that an analogous though not exactly equivalent charge could be laid against Lavoisier's preferred hypothesis for caloric as a material substance. In other respects caloric had significant differences from phlogiston: it was defined neutrally as 'the cause of heat', if it was a material substance it was always conceived to be imponderable, it did not impede the development of a lasting view of the chemistry of ponderables, and it could readily be dropped from a viable remaining theory. However, there would be considerable difficulty in maintaining that the theory change eliminated a circularity in this case.

Additionally, the protagonists of each theory considered that in principle their theory was empirical and not circular. Kirwan considered that an advantage of his version of phlogiston theory was that it made phlogiston isolable by experiment, and (as previously noted) his abandonment of the theory in 1791 was accompanied by the reason that he could not come up with an experiment to conclusively demonstrate the existence of phlogiston. Lavoisier's theory was also that substances should not be assumed if their existence could not be demonstrated, even though he ignored this in the case of caloric. Accordingly in this respect it is reasonable to put forward that there is no advantage to either theory.

Untranslatability and world changes.

Untranslatability was the key remaining item in Kuhn's pared-down view on incommensurability. However, in any comparison between languages, there are words in one language for which there are no words in another; also the intensions of words in different languages, whose extensions are the same, may be different. Translational failure is perfectly compatible with mutual understanding and also with rational appraisal, as is seen regularly with differing languages (Pyle, 2000, p. 117). Reference continues between theories despite the point; scientists have no difficulty with terms in different theory-languages, as exemplified by Lavoisier using such terms as 'dephlogisticated air', 'vital air' and so on. As Pyle notes (2000, p. 104), a very clear account of phlogiston was given by Lavoisier in his *Réflexions*; also, both Kirwan and Priestley had a rather good grasp of antiphlogistic theory: they anticipated how Lavoisier and his colleagues were going to respond to their objections. The objection to the new nomenclature was not that it was incomprehensible but that it was too orientated to the new theory. Accordingly, as Bird (2000) and Pyle (2000, p. 117) both concluded, translatability or the lack of it simply is not important as a factor in scientific change.

Kuhn's original view, that theory change involved changes to the world, has been discussed in many works; perhaps the most noticeable and lengthy feature of Hoyningen-Huene's 1993 book on Kuhn was his Kantian interpretation which attempted to save some of Kuhn's proposals concerning world changes. The key later discussion was by Bird (2000, pp. 97-149), who dealt at length both with how Kuhn's actual view can be seen to have arisen, and why it did not stack up; he concluded that "phenomenal worlds cannot be changed simply by a change in concepts". Pyle (2000, p. 104) also noted that "we can dismiss

Kuhn's talk of gestalt-shifts, different worlds, and mutual incomprehensibility as so much empty rhetoric".

Kuhn's successes.

Arguably the most influential of Kuhn's views was the view that at times of important theory assessment, it is generally the case that at least two theories are involved, not just one. In this view, a new theory will generally not be taken seriously until it is fully developed; this view of Kuhn's then explains why his view that crises, attacks and competing schools happen before the development of a new theory is generally wrong, and why in practice the new theory precedes crises, attacks and competing schools.

Hoyningen-Huene's argument that theory comparison is neither algorithmic nor irrational, is very watered down relative to Kuhn's statements in SSR, and is scarcely contentious.

Concerning the re-shuffling of entities within categories, and changes of vocabulary, it can hardly be any other than the Chemical Revolution with regard to which Kuhn developed these aspects of his theory – it is difficult to see that the points would be highly significant with respect, for example, to the Copernican revolution, in which while some entities changed categories, for example the Sun ceased to be a planet and the earth became a planet, there was no change in what they were called. It is correct that the Chemical Revolution resulted both in a re-shuffling of entities in the system of categories, and in some re-shuffling of categories and also some changes in vocabulary. The new chemical nomenclature was seen as a very important part of the system by both 'converts' and opponents, and it was seen as revolutionary by Lavoisier himself. However, Hoyningen-Huene goes on to say that the points about entities changing categories were important to Kuhn because they formed a foundation for his final view on incommensurability; it has been put forward that Kuhn's last view on incommensurability is a matter of no importance to the development of science, in which case even though the current point is correct it would also not be important in that respect.

Conclusions.

This paper has summarised many counter-arguments to Hoyningen-Huene's claim that the Chemical Revolution is an excellent example of the success of Kuhn's theory. While Kuhn's theory does have some relatively successful elements, in general it has been widely recognised as being necessary to 'beat nature into line' (Kuhn, 1962, p.135) in order to distort history sufficiently to fit into the "preformed and relatively inflexible framework" (1962, p. 24) of Kuhn's theory; this paper has clarified that this also applies to the particular example of the Chemical revolution.

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These will be inserted following the blind review process.

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Kuhn and the Chemical Revolution: a Re-assessment

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